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Dear Jim,

I enclose some very rough notes on your manuscript. I hope you will excuse my not revising them further, but Odile is unfortunately still in hospital and I have been very pressed for time. I am keeping the original until I hear from you where you want it sent.

Yours,

pp F. H. C. Crick

In what follows I shall comment mainly on what I regard as factual errors or omissions in the manuscript. This should not be taken to imply that I agree with the remainder of the manuscript - there are quite a number of judgments which I believe to be false which are not strictly matters of fact. For example, Chapter I, page 2, you say "Sir Lawrence Bragg was not used to telling people that he could not follow the argument". I think this is unfair to Bragg because it is one of Bragg's characteristics in a colloquium that when he did not understand what was said he would say so. This is not always the case with senior people but it was very characteristic of Bragg. I think the fact that he did not always like the way I put arguments should not be generalised to say that he was slow to state when he could not follow an argument. I also think it highly unlikely that he came only infrequently to tea was due to anything to do with me. It seems to me that this is just a guess on your part. On a point of detail, it is not true that I twice flooded his office with water, since in fact his office was on the opposite side of the lab to the room in which I worked with water; although it is true that I did twice cause a flood it was not due to the rubber tubing around a condenser but the rubber tubing round a suction pump.

In a similar vein, on the next page you imply that the Fellows of Caius did not enjoy my company because of my laugh.

I doubt if you have any evidence for this since in my early days at Caius I was as quiet as a mouse. I think you are just guessing, but of course I may be wrong. However, these are minor points. The first thing I strongly object to is the beginning of Chapter 2, when you imply that I accuse Bragg of "stealing" one of my ideas. This really does not correspond to what happened. Bragg had the idea quite independently in a somewhat different form; I merely said that it was not a new idea, but I certainly never accused him of actually stealing it. I was not at all worried about priority, I was more worried in fact that the idea had not been used before. John's version is that Bragg was mainly upset because I said I would think about Bragg's idea and tell him whether I thought it was right or wrong. In fact, as it turned out, it was not completely right ~~but Bragg was not to know that at the time.~~ My formulation of the problem was the more exact one, although Bragg's was quite a graphic one.

On further points of detail, page 2, Chapter 2, it is not in fact true that a German bomb fell on the lab at the beginning of the war; in fact the lab was closed at the beginning of the war and it was then I joined the Admiralty; it was at a later date that a German bomb actually fell on the lab. In the same way, it was not really C. P. Snow who had much to do with my troubles with the Admiralty. I don't think the exact story is of

much interest - but it was roughly as follows: the first time I was interviewed for ^{a permanent job in} the Scientific Civil Service by three provincial professors they rejected me. The Admiralty, however, were so keen to have my services that they arranged for me to have a second interview and it was on this occasion that C. P. Snow was head of the interviewing board. I did not produce a very good impression but they nevertheless decided to keep me on. However I then made up my mind that I wanted to leave and I approached Massey and through him was introduced to A. V. Hill, who was in fact the major influence in getting Mellanby to give me an M.R.C. studentship.

In Chapter 3, page 2, you say "Michael, then at school, was looked after by his mother and aunt". The "his" of course really refers to me and not to him, but the sentence is not clear.

Chapter 3, page 3; at least one of the reasons for my lack of enthusiasm for politics was the fact that I had been, after the war, a Civil Servant at the Admiralty in Whitehall and had seen something of the inside of government and I formed the opinion that it was not a very interesting thing unless one was especially informed about what was going on. It was for this reason that we have never had a daily paper since then and I never read The Times at breakfast as you imply.

Your account of what happened with Bill Cochran and the

diffraction of the Melix is right in outline, but is wrong in quite a number of details. What actually happened was that I had a headache that day at lunchtime at the Eagle and went home instead of going to the laboratory so that I would get rid of the headache in time for the wine tasting. When I was sitting at home in front of the gas fire I got bored and started to work on the problem which I had discussed with Cochran that morning. As far as I recall I finished all the algebra before I went to the wine tasting and your wonderful generalisation about the absence of women
/ing
bring me luck I don't think has any foundation in fact at all.

"
Nor is it correct to say that Bill's equations were more direct and gave easy numerical solutions in contrast to Francis' more laborious approach."

"
The fact is that Bill's derivations was much more elegant than mine but the two answers, apart from the fact that each of us made a trivial slip in sign, were identical.

Where Bill had the advantage over me was that he had a table of Bessel functions and he should get the credit for pointing out that the Bessel functions were of such a shape that they enabled predictions to be made from the X-ray diagram. I would have done this as soon as I had seen the Bessel functions but I had not had time at the moment when he came into my office. I like the phrase "bounded up to Cochran's office" - in fact I was sitting down when he came into mine. At the end of the chapter you say

it was jubitantly dispatched to a crystallographic journal but it was in fact sent to Nature. A longer paper, of which the three of us were co-authors, was sent to Acta Crystallographica at a later date.

My criticism of this chapter, which is partly finicking details and partly matters of some importance, is typical of what I feel about the manuscript as a whole - namely that you have got the thing right in a sort of way, but that it is a distortion of the facts if one looks at it carefully. To come to Chapter 4 I cannot now remember what I thought at the time about the relative importance of proteins and DNA but I don't think it was quite as clear in my mind as you make out. However you may recall this better than I do. I think I was interested in DNA but I did not fret under the restriction that it belonged to Maurice Wilkins; I was more concerned at the time that he should get on with the job. Incidentally, is it really true that Rosalind was Maurice's assistant as you imply on page 3 of Chapter 4. I don't think in England one ever uses the term "hired" in the phrase "Maurice ever hired Rosy", and in any case I wouldn't be surprised if she was engaged by Randall. Altogether I feel that some of the wording in this chapter is a bit too strong - words like "idiots", "cantakerous fool" and "the situation was thus idiocy" gives the whole chapter a too hysterical feeling for my part. I have very few comments on Chapter 5

which is mainly about yourself, but I seem to recollect, page 6, that Frank Putnam or somebody else did the same experiments as you at about the same time and it would be polite to put in just a mention of that if this is indeed the case. For the same reason I find I have very little to say about Chapters 7 or 8. In Chapter 9, page 6, I think you don't get the position of Stokes quite clear. Stokes had actually worked out quite independently of Cochran, Vand and myself the theory of helical diffraction. There is no question of saying Stokes' work is not air-tight, it was just as good as ours. However, as you know, the theory is not enough to prove that a given picture represents a helix and that was where the doubt lay. It is certainly true that Maurice or Maurice and Stokes were the first people to realize that DNA might be a helix. In Chapter 10, on a point of detail, I didn't know that Rosy was at Cambridge, although this may well be true - I think you should check it.

In Chapter 11 your account of our visit to Oxford seems to be substantially correct. I had forgotten that this was the occasion that we say Kreisler^e, but I have no doubt that you remember this better than I do. Chapter 13, page 2, you may not want to put it into your manuscript but the scientific reason that we got the water content wrong was that you told me there were three or four molecules of water in the unit cell

whereas what Rosy had said was there three or four/molecules of water per asymmetric unit. Chapter 16, page 5; Odile has a small comment. You say that she was keen to attend the Tropic Night Ball since it was sponsored by black people. She wants to point out that she attended because she was asked to do the decorations and not because of some colour prejudice on her part!

Chapter 18 - I had completely forgotten that you had told me about Chargaff's results before Chargaff himself came to Cambridge and before I talked to John Griffiths. I think the likely explanation is that you did tell me but it made no impression at the time. Otherwise I am quite certain I would have remembered it when John Griffiths told me about his calculations. On page 3 you say that "Griffiths did not go along since for some months he had preferred to scheme where gene copying was based on the alternative formation of complementary surfaces". This may well be true but I can only say that he did not base say this to me at the time. At the time I suggested that like bases would ^{at} detract each other by stacking one on top of each other and asking if he could do the calculations. I was therefore very surprised when he told me later in the tea queue one afternoon at the Cavendish that he did not get an attraction of like with like but he did get that adenine should go with ^{thymine} ~~cyanine~~ and guanine with cytosine. It was at this point that I said to him that this would immediately

give complementary replication. He did not make the remark to me although looking back on it it must be obvious that he thought of the idea for himself as well. I did not realise at all at the time the implication of this result was that you should have 1:1 base ratios although it was very stupid of me not to have thought about it. It is true that I doubted the exactness of John Griffith's arguments and especially the magnitude of the effect since I was able to make a rough estimate of that myself using simple electrostatics. As I recall it I first heard of the 1:1 ratios in any way that made any impression on me when talking to Chargaff in John Kendrew's rooms. I had not remembered it was after dinner, I thought it was in the afternoon, but that doesn't matter. I certainly haven't the slightest recollection of mentioning John Griffith's results to Chargaff, although I may well have done so. It is true that I did not at that time know which of the four bases was which, but the fact is that I couldn't remember the names that John Griffiths had told me. However it is possible that I mentioned them to Chargaff and have since forgotten. For the same reason I did not forget Chargaff's results in the embarrassment of the situation; I simply forgot them because the names of the four bases didn't mean anything to me. Incidentally, you have omitted from your account that somewhere about this time I did a week's experimental work trying to prove

there was a force in solution between nucleosides to give the adenine - cyanine - guanine - cytosine attraction but that these experiments failed because the effect, if any, was too small for me to pick up by the technique I was using. I still have these results in a notebook somewhere. Incidentally, I had a lot of trouble convincing you that Chargaff's rules with the 1:1 ratios did mean complementary replication, although eventually you came round; I think you only really came round after you saw some early accounts of J. Wyatt's work, although I am not sure of my recollection of this point. I can't recall at this time whether I ever did discuss with John Griffiths the 1:1 ratios, as far as I know I was the only person in the world at that time who realised that 1:1 ratios meant complementary replication.

Chapter 22; I don't think it is true that Bragg put ~~all~~ *Pauling's* ~~his~~ manuscripts aside. What he actually did was to ask Max and John to go upstairs and discuss it with him at the same time as we were talking with Peter downstairs. Incidentally, you never mentioned, in discussing Pauling's model, that he used an old photograph of Asprey which had both the A and the B diffraction pattern together, so that he in fact solved a structure which never existed in any real sense at all. I think this is so, but perhaps you should check the original paper. There is quite an omission somewhere in this story in that sometime in the summer before Linus's model came out you and I, or at least I, had a

longish talk with Rosy in the tea queue at the Zoo lab at some conference or other in which she firmly maintained the structure was not helical and I maintained that it was certainly likely to be helical and that she should scrutinise the evidence which appeared to be against it very carefully.

Chapter 24, page 5; the main reason you gave at the time for putting the phosphates on the inside was the extraordinary one that the long chains of the lysine and arginine ^{from} ~~for~~ the ^{proteins} ~~proteins~~ could then ^{reach} ~~breathe~~ inwards so that their basic groups would be against the phosphates. I was always absolutely unmoved by this argument. At the end of the conversation with you as you rightly imply I asked you why you did not try building models with the phosphates on the outside. Your reply was that it would be too easy, to which I replied, as you went up the steps, "then why don't you do it?". I find at this point a major omission in your account of the model building. If you recall, as you rightly say, your first started off by putting like bases with like. This meant that there would be a dyadaxis parallel to the helical axis and that the angle between residues would be 18° and not 36° . You were tempted to build models with an angle of rotation of 18° but you were always unsuccessful and asked me, just before you went out (to play tennis[?]) whether I would do it. I quickly convinced myself that an angle of 18° was impossible and I built a model for you with an angle of about 36° which looked quite

reasonable. I also had difficulty at this time in getting across to you the importance of the space group of the A form which was C 2 and which therefore clearly implied *dyads* at the side. You did not like this argument at all but in the upshot, as you know, I was right. It was for this reason that I was very happy at building a model with an *angle* of 36°.

Chapter 25, page 2; in your account of the manuscripts that we got from the M.R.C. Committee you should bring out the point that Rosy stated categorically in that manuscript that, (I think you will see if we look it up) the structure was not helical and Maurice, who had a separate contribution, reluctantly followed her example. I think it would be sensible anyway to try and dig up this manuscript and find exactly what it contained.

Going back to the previous Chapter 24, page 6; at the bottom "Maurice's slow answer emerged as 'No'". This is actually slightly ambiguous. You should make it clear that he when he said "No" he meant not that we couldn't do it but that he would not mind if we played with DNA molecules. This does not come over completely clearly in the way you have written it. Another omission about this period I think is that you should make clear why, although we knew that 1:1 ratios meant complementary replication, we did not incorporate it in the model building. Our reason was that we decided to reject anything in the preliminary

model building which we were not completely sure of and we could not be completely sure that this was not due to some other reason. It was only after we had decided to put the backbone from the outside and you had explored the like pairing that the astonishing idea dawned on us (and I remember very vividly the particular moment) that you might be able to get complementary replication by making unlike base pairs and having the backbones run in opposite directions. It was the day after that that you came in, as I recall, having correctly put the base pairs together. In other words, I thought that we had realised that we should use Chargaff's rules before you made the base pairing and that you then looked for them and found them. However, it may be that my recollection of this is not quite correct. There is one technical detail that you have missed in Chapter 27, although this is not of great importance and we never published it. This was that at about that time I proved a geometrical theorem so that I did not have to build both backbones at once but could work on one half of the base pair in refining the model. This made the work of refinement very much easier.

Chapter 27, page 4; you may be right about Bragg seeing the model, but my recollection was that when we got the structure he was having 'flu and that Eric Howl^es went along to see him and told him that we had got the model and that he only actually saw

it a little later. Incidentally, I was so tired after the three or four days solid model building, during which, if you recall we tried at least two different variants of the model, that on the Saturday evening when we had all finished and I had got the co-ordinants I retired home and went to bed.

Chapter 28, page 4; I don't think Todd came over with several younger staff - as I recall it it was just Dan Brown.

Chapter 29, pages 1 and 2; I think if you look up Jerry Wyatt's paper you'll find that he did say that 1:1 ratios could meant complementary replication, or something of this sort, although he said it rather cautiously. I think that if you are going to mention him you should bring in this point to do him justice. Incidentally, it was while you were away in Paris that we both independently thought of the mechanism of the rotation by shift of the tautomeric hydrogen in the base pair. I thought I drafted the sentence which began "It has not escaped our notice ." and I remember we had to defend it from criticisms by Max or John or someone like that, but it is a matter of no importance. Incidentally, one of the bizarre things about our paper is that the editor would not allow us to use the initials DNA and so we constantly refer to "the acid" in our paper although we had originally written DNA. I am not quite sure if this was our first paper or our second paper, but we can easily check that. You say Linus arrived in Cambridge on a Friday night, my recollection was

that it was Good Friday and in fact the lab was officially closed on the day that we had our meeting.

Reading your manuscript I cannot help remember the lecture which I gave on the subject some years ago, first at Cambridge and second at Oxford, to societies interested in the history and philosophy of science. The difference between my lecture and your book is that my lecture had a lot more intellectual content and nothing like so much gossip. Yours makes a good story, especially as it gives a rather vivid picture of what you were up to at the time, but what I miss in it is the intellectual conclusion that can be drawn about *our work*.

I don't know whether I should write this up in ~~some~~ form or another since it could be comparatively short. Of course there was some gossip in my lecture but only just a little bit to alleviate the scientific arguments. Your book on the other hand, is mainly gossip and I think it a pity in this way that there is so much of it that it obscures some of the important conclusions which can be drawn of what we did at the time. There are quite a number of fallacies going around about the way we did our work and although your manuscript enables one to see through some of them they are not brought out clearly and refuted as they might be in a more sober treatment. I think it would be a good idea if you had a glossary of some sort of the people involved, especially as you have two Maxs, Max Perutz and Max Delbruck, and it would

help people who do not know the characters to follow through if at the beginning there were a list of the main characters with short notes as to who they were.